JOURNAL OF THE EXPERIMENTAL ANALYSIS OF BEHAVIOR 2004, **82,** 95–102 NUMBER 1 (JULY)

MODERN MOLAR BEHAVIORISM AND THEORETICAL BEHAVIORISM: RELIGION AND SCIENCE

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Baum and Staddon disagree on the status of internal states in behavior analysis. Baum advocates molar behaviorism, treating behavior in temporally extended segments and so avoiding the need for internal states. Staddon argues that internal states merely represent the effects of different histories and that their use brings behavior analysis in line with the established sciences. The dispute is one form of the age-old molar–molecular controversy that characterized Aristotle's disagreement with Plato. Both molar and molecular analyses have their place, but molar behaviorism may apply more naturally to a variety of phenomena, ranging from the matching law and avoidance learning to so-called ''higher mental processes.'' When molecular analysis involves internal states, as in Staddon's Theoretical Behaviorism (or New Behaviorism), misunderstanding will be inevitable and behaviorism will be seen as one more instance of the mediational theories in which psychology has long been mired. Such theories have long dominated the physical sciences, where their usefulness is indisputable, but psychology is far behind the physical sciences and nonmediational molar behaviorism better suits a discipline that lacks the methods and the data of the established sciences.

Key words: theoretical behaviorism, molar behaviorism, molecular behaviorism, internal states

''I like John Staddon; I just don't like his book!''

(W. M. Baum, personal communication, May 25, 2002)

''I thought behavior analysis was science, not religion, but maybe I was wrong.''

(Staddon, 2004, p. 83)

The Baum and Staddon dispute is an important one and must be viewed in historical context—it goes back at least to Skinner's classic writings of the 1930s. Perhaps it even goes back to Aristotle's departure from Plato's philosophy. The debate may be seen as a conflict between an ''old'' and a ''new'' behaviorism. But *which* is the old and which is the new? The disputants disagree.

In what follows, I will consider the main ways I see the views of Baum and of Staddon differing, namely concerning: (a) modern molar behaviorism and its pedagogical advantage; (b) the application of basic research to societal problems; (c) what really constitutes ''theoretical behaviorism;'' (d) the revolutionary nature of modern molar behaviorism; (e) whether the goal is understanding animal learning or human behavior and experience; (f) the virtue of molar behaviorism; and (g) molar behaviorism and mental states. I will close with a comment on the dangers of *The New Behaviorism* and a conclusion.

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MODERN MOLAR BEHAVIORISM

I am proud to have been a student of John Staddon and I feel great affection for and intellectual loyalty to him. But I will admit at the outset of this essay that I sympathize with Baum's molar rendition of behaviorism and am skeptical of the rationale supporting Staddon's theoretical behaviorism. I confess that this sympathy and skepticism come in part from years of convincing undergraduates that Skinner's radical behaviorism is not the old S-R psychology typically portrayed in textbooks. It is easy to show that molar behaviorism is not that old caricature, but I argue that Staddon's theoretical behaviorism constitutes a variation on Clark Hull's theory, which was the very epitome of S-R behaviorism. That molecular behaviorism (Hull called it ''molar'' in 1943) aligns with much of the rest of psychology, especially cognitive psychology. As I pointed out in 1987, if you insist on discrete, proximal causes, then you need mediators, whether they be cognitions or habits. But Baum's modern molar behaviorism aligns with no other modern psychological views and so remains largely unknown.

I also teach that behaviorism can deal plau-

sibly with what most psychologists think of as ''higher mental processes,'' but it does so in the way that Aristotle did in his *Nichomachean Ethics* (4th CBC/1943), as I will discuss below. Molar views like Baum's lend themselves to teaching behaviorism as a plausible and appealing account for many common psychological phenomena. Staddon's behaviorism does not have that virtue. This does not make Baum's molar behaviorism *better* as the basis for a scientific psychology—indeed, Staddon's theoretical behaviorism is more in line with traditional science, as most of us understand it. But pedagogical and public relations values should count, especially when teaching and dealing with colleagues who were taught that behaviorists believe that humans are ''machines'' and that ''the mind is not worth studying.''

Baum's response to Staddon's (2001) *The New Behaviorism* and Staddon's reply indicate a clear difference in visions of a behaviorism held by the two. There is a *real* difference here, and I am unable to take one or the other side unreservedly. Baum espouses what he calls *modern molar behaviorism,* a view emphasizing the analysis and value of treating behaviors extended in time. Modern molar behaviorism stems from Skinner's early (1931, 1935) writings, and examples of its applications to cognitive and other phenomena are supplied by Rachlin (e.g., 1994). For example, ''love,'' ''intelligence,'' ''memory,'' ''intentions,'' and other cognitive/emotional terms are made intelligible when described as *temporally extended patterns of behavior,* just as a fixed-interval scallop may describe the overall pattern of responding under a fixed-interval schedule. Molar behaviorism is thus an alternative to traditional cognitive psychology and it is absolutely antithetical to it—cognitive psychology, by definition (e.g., Neisser, 1967), always invokes mediators and molar behaviorism never uses them. Staddon's theoretical behaviorism, like Hull's (1943) system, is of the same stripe as cognitive psychology (see Malone, 1987, where I identified and admonished the promoters of the then popular ''cognitive revolution'').

The molar perspective has offered useful approaches to several salient puzzles that have persisted in the research literature. These include, for example, those variables controlling avoidance conditioning (Herrnstein & Hineline, 1966) and matching (e.g., Malone, 1990). Despite these successes, however, in each of these cases there are competing molecular accounts (e.g., Catania, 1998; Dinsmoor, 2001).

Modern molar behaviorism also is illustrated in Baum's example of Tom's uptown bus ride, in which purposes and intentions appear in his temporally extended behavior, rather than as internal states or causes of observable behavior. As indicated earlier, an emphasis on temporally extended behavior is reflected in Aristotle's *Nicomachean Ethics.* Over two millennia ago, Aristotle proposed that virtue, love, honesty, happiness, courage, and other ''traits'' and conditions were actually patterns of activity persisting over weeks, months, and years. Aristotle was no behaviorist, to be sure, but this approach is ''made modern'' when translated into behavioral terms.

For most psychologists, students, and the public, ''behaviorism'' means nothing more sophisticated than the simple treatments found in introductory textbooks. And those treatments *always* present behaviorism as a molecular view, that is, as an S-R simple associationist psychology. Baum was no doubt alarmed and concerned when he saw that Staddon's book appeared to advocate old-time molecular S-R psychology and, in addition, diminished the importance of molar behaviorism by substituting internal states for temporally extended patterns of behavior. Even more troublesome perhaps, Staddon's book is stylistically a delight to read, full of anecdotes and insights—many that seemed to have little or nothing to do with theoretical behaviorism but lend support to it by association. Some readers must think that if such a writer is so compellingly convincing in his critiques of Skinner, cognitive psychology, cognitive neuroscience, and applied psychology, he must therefore be correct in his advocacy of theoretical behaviorism—of course, we *need* ''internal states!''

The Application of Basic Research

Although I suggest that molar behaviorism is more plausibly applied to the understanding of what is usually called ''higher mental functions,'' that is not to imply that it is necessarily a more useful approach for application. When it comes to topics like behavior

therapy and social problems, we find Baum and Staddon disagreeing again. Baum advocates extending the application of behavior analysis toward solving complex human problems, whereas Staddon vehemently argues against this kind of effort.

I have used Baum's *Understanding Behaviorism* (1994) along with Staddon's (1993) earlier edition and concluded that Baum was all too ready to extrapolate from the laboratory to the important problems of society, as was Skinner before him. For example, Baum's Part Three, ''Social Issues,'' deals with credit and blame, interpersonal relationships, management, government, and religion much in the way that Skinner did in *Science and Human Behavior* (1953), *Beyond Freedom and Dignity* (1971), *About Behaviorism* (1974), and in many other writings.

Like Skinner, Baum's applications tend to be vague, but even when they are specific they exemplify the old molecular behaviorism, because such approaches tend to be successful in education, behavior therapy, and elsewhere. Rather than temporally extended behaviors, we find reinforcers, discriminative stimuli, discrete behaviors, shaping, and the rest of the jargon of an old-time behaviorism. Of course, all of these terms need not denote molecular things, and they did not in Skinner's early writings (e.g., 1931, 1935, 1938). But in the applied literature they are invariably used in a molecular way, indistinguishable from Hull's usage of similar terms. I would love to see exceptions to this and I am sure that they exist, but in my examinations of applied literature, I have not been able to find them. Applied behavioral methods might as well have been inspired by Hull as by Skinner.

That said, I am in agreement with Staddon and am skeptical, for example, of the value of behavior-analytic applications, given our current state of knowledge, though I am sure that even he would admit that behavioral methods have a useful, albeit limited place in education and behavior therapy, especially when contrasted with other available methods. However, the application issue has nothing to do with the merits of molar behaviorism or of the theoretical behaviorism that Staddon advocates when it comes to *understanding* behavior—successful application does not guarantee understanding. So, what *does* Staddon advocate?

What is ''Theoretical Behaviorism?''

Staddon's ''New Behaviorism'' and its ''internal states'' invites, even assures, misinterpretation, because the states will inevitably be construed by readers as ''events inside the organism.'' This despite all of Staddon's good intentions and his assurances that his states merely refer to histories of contingencies—equivalent states are the bases for equivalent behaviors, but, certainly, behaviors that seem equivalent may arise from very different histories. The rest of psychology takes internal states for granted, as cognitions, wishes, memories, sensations, and much more. So this means that theoretical behaviorism will join the dreary mainstream of psychology and, worse, behaviorism with internal states will mean that Clark Hull will be reincarnated—''Theoretical behaviorism is a natural descendant of both classical and Hullian behaviorism'' (Staddon, 2001, p. 142). I am compelled to quote the following:

> Nothing is better evidence that Hull's influence remains than this indirect recognition by his detractors that it is still a force that could regain strength if the fashion of the moment were to fade or become less appealing. (Amsel & Rashotte, 1984)

The Revolutionary Status of Modern Molar Behaviorism

Staddon ended his objections to Baum's review by implying that what Baum promoted was *religion,* not science. This assertion highlights the distinction between these two leading figures in modern behaviorism. Is behaviorism a system of thought, encompassing epistemology and ontology, and is behavior analysis *the* science of psychology, as Baum (1994, pp. 25–26) argues?

> Modern radical behaviorism is based on pragmatism. To the question, ''What is science?'' it gives the answer of James and Mach: Science is the pursuit of economical and comprehensible descriptions of human natural experience (i.e., our experience of the ''natural world''). The goal of a science of behavior is to describe behavior in terms that render it familiar and hence ''explained.''

If one holds this view, then the science of behavior should be applied as widely as possible

and used to inform and reform other visions of psychology. That is Baum's position, and it may have much merit in dealing with persisting problems in the literature and with traditionally cognitive matters as mentioned above. Radical behaviorism as a revolutionary movement arouses fervor—similar to religious enthusiasm—in many of its participants. But the revolutionary fervor of such a position provokes premature efforts to apply it as widely as possible.

Staddon does not share Baum's vision of revolution. For him, Skinner's radical behaviorism is only one of several incarnations of a more or less proper scientific approach, but he strongly emphasizes that *radical behaviorism has essentially forbidden legitimate scientific theorizing*. Worse, modern behaviorists like Baum follow Skinner in recommending extended application to mental health care, education, administration of justice, business practices, social welfare, and elsewhere. This is anathema to Staddon, who views much (but not all) such application as the work of self-appointed gurus, to be classed with management consultants, motivational experts, and other hucksters.

> Skinner's besetting sin is one that he shares with all social science gurus: premature extrapolation. His brilliant and innovative laboratory research encouraged him to take a tenuously based and in some ways half-baked set of social prescriptions to a public willing to accept them because of the very real laboratory successes of operant conditioning. He offered false arguments about personal responsibility and the supposed inefficacy of punishment that reflect the way he would have liked the world to be, rather than the way it actually is. (Staddon, 2001, p. 122)
>
> From Watson to Skinner, whether they studied rats, pigeons, or people, the leading behaviorists were all preoccupied with practical results. Their facts were at least real facts: running speed in a maze, key pecks in a Skinner box, and so on. But their interests were utopian—to change the world, not to understand it—so they vaulted from fragmentary knowledge to sweeping recommendations about social policy and private action. Its weak philosophy and grandiose claims made behaviorism a soft target, even for the poorly guided missiles directed against it by Chomsky and other critics. (Staddon, 2001, p. 179)

I sympathize with Baum's attempt to spread "the good news" (e.g., 1994) that behaviorism can be a molar enterprise to encompass all of ontology and epistemology. But application of the science to a plethora of society's problems is another matter, and we should not aspire to be social engineers until we can agree on what society *should* be. As Staddon argues, even issues that seem beyond debate, such as those ills produced by cigarette smoking, may not be so simple. He noted that it is conceivable that cigarette smoking actually benefits a society in several ways (p. 89), though most would argue that we should eliminate cigarettes as a public health measure. Naturally, we can and should deal with the autistic child, the disruptive student, and the phobia sufferer; successes in these domains and others such as instruction are most impressive and clearly significant. However, I agree with Staddon that we are not competent to deal with complex cultural issues such as the justice or welfare systems. My favorite quotation along these lines comes from his 1993 book:

> It is scant comfort that his [Skinner's] sin in this respect is no greater than the sins of all those other over-confident social scientists—psychologists, psychiatrists, sociologists, economists—who continue to go beyond their narrow, particular and often ideologically driven understandings of human nature and society in 'expert' testimony of all kinds. They should keep silent, or at least show a decent modesty in the face of our enormous ignorance. Human nature is stranger than we know—and stranger than even we can imagine. In vital matters like marriage, raising children, and the punishment of crime it will be many many years before the one-dimensional pronouncements of 'experts' can be reliably trusted over traditional wisdom and personal experience. (Staddon, 1993, p. 83)

Is the Subject Human or Animal Behavior?

Baum is interested in animal behavior as well as human behavior, the latter including what is usually called private behaviors (e.g., thinking, imaging, and the like) and what is usually called cognitive activity (e.g., reasoning and planning). Following Skinner, Baum does not hesitate to extrapolate or apply the findings and principles derived from animal experimentation to the most complex human behavior. By contrast, Staddon, as a behavior-

al scientist, is interested almost exclusively in *animal behavior,* as he expresses at the beginning of the book and at its closing:

> The audience for this book is graduate students and advanced undergraduates interested in the psychology of learning, especially animal learning. (p. xiii)
>
> Don't reach for the stars; reach for a telescope. Study the dynamics of simple animal behavior. Maybe the stars will arrive in due course. (p. 180)

Indeed, Staddon's theoretical behaviorism is currently deemed applicable *only* to simple phenomena, where data are clear, the sort of data gathered more often in animal studies. Otherwise, he appears to be interested in human behavior only to the extent that would be true of any other intelligent person, whatever their profession (pp. 73–74).

The Virtue of Modern Molar Behaviorism

The *temporal* molar view must not be confused with the old molar learning theories, such as Tolman's, that were molar with respect to *space.* Tolman proposed mediating representations of an organism's environment and significant features therein, for example, cognitive maps, to account for many aspects of learning. The molar behaviorism to which Baum refers is quite different and the question involves what counts as units of behavior. The answer is the one given by Skinner in 1931—units of behavior and environment ("S" and "R") are defined where orderly relations obtain between them. But such order may involve behaviors extending over considerable periods of time. Never mind that the question of "order" is an esthetic one. Because patterns of activity extended in time are fundamental, there is no *necessary* reliance on discrete, contiguous causes. The issue of order necessarily involves selection of an appropriate level of description—molecular analyses may always be possible, but molar descriptions may be more informative, depending on the behaviors in question and our interest in them.

Skinner (1931, 1935) defined stimuli and responses as empirically revealed functional classes that are related in an orderly way—these classes may be molecular–discrete or molar–extended in time. Perhaps phobic reactions or learned taste aversions are best described in molecular terms, as simple associations among events. However, the law of effect may be best treated in molar terms, for example, as described by the matching law, where the "response" is a class of behaviors extended over time ("choice") and is related to another class of environmental events (relative reinforcement rate). Staddon has suggested (personal communication, July 2002) that behavioral histories may be similarly defined as "classes," so that internal states assume the same status as stimuli and responses—functional classes to be described as models. My concern is that histories represented as cascaded integrators and feedback loops appear to return us to a Hullian past.

Molar Behaviorism and Mental States

Tom's boarding the uptown bus is not *caused* by his belief that it will take him home—his boarding the bus *is* that belief, or part of it, and it is not a "covert response" that precedes overt action. The situation is as Aristotle might have described it:

> For Aristotle, the idea would not be a covert response at all but a pattern of wholly overt responses including the individual's verbal report as one part of the pattern. (Staddon, 2001, p. 138)

But Staddon rightly insists that satisfactory explanation in science often requires reference to contiguous events ("causes") and that molar interpretations have no exclusive, or even favored, status (2001, discussion on p. 138). Molar explanations rely on histories of exposure to contingencies, that is, "histories." Theoretical behaviorism refers to "*states*" to represent those histories. Equivalent histories lead to equivalent states, not *within* the organism, but as *summaries* of histories.

> I will argue in a moment that this way of dealing with internal events as temporally extended patterns of stimuli and responses amounts to defining internal states in terms of equivalent histories. (Staddon, 2001, p. 138)

So he argues; whether others will interpret him this way remains to be seen, especially when they read his page 142, where his kinship with Hull is clear.

> Theoretical behaviorism (TB) is a natural descendant of both classical and Hullian behaviorism . . . TB sees internal states as purely theoretical constructions based on informa-

> tion from historical experiments. Nevertheless, it shares with Hullian behaviorism the idea that the ultimate aim of behavioral study is the derivation of *mechanisms* . . .

On page 144, a section is headed, ''The Model is the Behavior.'' That is the essence of Hull's behaviorism.

If one must teach classes wherein behaviorism is discussed, modern molar behaviorism is a godsend! Descriptions of extended patterns of observable behavior, and the contingencies that maintain them, are both interesting and satisfying to students. But the awkward language of operants, discriminative stimuli, and reinforcement (to say nothing of the ''three-term contingency'') has never lent itself to the description of many psychological phenomena, such as perception, memory, intentions, and more (cf., Malone, 1987). In addition, such language has not dispelled and perhaps has even promoted the mistaken belief that radical behaviorism is essentially the same as any old S-R learning theory. Unfortunately, this belief has been responsible for misguided attacks over the years (the ''poorly guided missiles'' to which Staddon referred on p. 179 of his book). ''Discriminative stimulus,'' for example, can easily be interpreted as meaning what Clark Hull meant when he wrote of ''stimulus energies'' (e.g., 1943). A discriminative stimulus is almost never interpreted as a *class* and the same misinterpretation holds for ''operant.''

Molar behaviorism allows us to include everything that is commonly called ''mental,'' ''cognitive,'' ''ethical,'' and all the rest of the conventional psychological categories. Modern molar behaviorism not only rescues us from sole reliance on the language of stimuli and responses, but it unites us with a monist epistemology, one that could be endorsed by Aristotle, Protagoras, Hume, Peirce, James, and Quine (cf., Malone, 2001; Malone, Armento, & Epps, 2003).

Marr (1997, p. 77) offered a different perspective in his description of dynamical systems. Many systems, ranging from tornadoes to double star systems to reinforcement contingencies to biological evolution show large-scale (i.e., molar) behaviors emerging through nonlinear interactions of sometimes relatively few controlling variables. Complexity can indeed emerge from simplicity. In many, if not most, examples the mechanisms for emergent phenomena are unknown. In such cases, as perhaps in some of the patterns of molar behavior Baum describes, emergent order may be difficult to explain, if by explanation one means translation into proximal discrete mechanisms.

The Dangers Inherent in The New Behaviorism

Staddon's internal states keep us bound to what virtually everyone will view as contiguous *causes* for relatively simple behaviors. Most readers will conclude that the internal states are causal mediators like other proposed mediators (habits, cognitions, sensations, and so on)—despite the fact that this is not Staddon's position!

As I indicated previously, the insightful, charming and witty pages that constitute the bulk of Staddon's book have little to do with theoretical behaviorism. Many of the issues that he so entertainingly and insightfully presents are framed as trenchant commentaries on Skinner's arguments and speculations, as well as those of his sympathizers. For example, Staddon's convincing arguments for the value of appropriate punishment in the justice system (chap. 5) are in contrast to Skinner's general opposition to the use of punishment. I believe that Staddon is certainly correct here, but his views on this topic really have nothing to do with theoretical behaviorism. In fact, as already emphasized, Staddon, on the one hand, mostly shows that Skinner's proposals for the improvement of society are open to question—the ideal society is not a given but is a matter for debate. Baum, on the other hand, seems to agree with Skinner, assuming that we all know what society should be like and we can, with the proper application of behavior analysis, bring it about.

Staddon is careful and sincere in his attempts to bring behaviorism into the mainstream of *science*—but *not into the mainstream of psychology*—the wasteland that Staddon described:

> Biologists can show off vaccines, antibiotics, cloning, and other marvels. The by-products of mind-science are rather less impressive: cognitive behavioral therapy, Thorazine, Prozac, shock therapy, alleged genetic markers for homosexuality, IQ tests . . . Theories of human nature never really die; they just go in and out

> of fashion. (Horgan, 1999; quoted by Staddon, 2001, p. 180)

That wasteland is where unrestrained speculation on internal states can lead us.

Molar behaviorism is apparently unknown to the rest of psychology, which is absolutely awash in agency, internal states, and other mediators of every possible description. So, considering all of psychology, Baum's message is still largely a secret. No wonder Baum reacted so spiritedly to *The New Behaviorism*—it seemed to him to endorse everything that molar behaviorism battles against.

Here is the danger: Many psychologists who would certainly deem themselves as good scientists are perfectly comfortable with explanatory mediators and thus may view Staddon's *The New Behaviorism* as an endorsement of their own views. In most cases, they will be mistaken because Staddon is not arguing for mediationism per se—but they may well take his book as an endorsement (or, worse, admission) ''even from the radical behaviorists'' of the necessity of mediational theory.

Conclusion

Baum's commentary was not really a review of Staddon's *The New Behaviorism*; rather, Baum reacted to what he perceived as a reprise of mediational behaviorism. The theoretical behaviorism that Staddon promotes is easily interpreted as a molecular view where hypothetical internal mechanisms abound and one imagines Clark Hull smiling in approval.

Baum and a few others have promoted an alternative molar view that is reminiscent of both Aristotle's philosophy and Skinner's 1931 and 1935 papers. This molar behaviorism is not known to the public at large or even to most psychologists, judging from the absence of reference in journals and textbooks. Staddon described two ways of approaching behavior in his response to Baum:

> So there are two choices: either define ''behavior'' in increasingly abstract historical ways so as to accommodate its real complexities, or stick close to something like the physical definition, and permit proliferation of state variables. Baum favors the former, I embrace both, but tend to favor the latter. (Staddon, 2004, p. 82)

Patterns of behavior over time need not be abstract and historical, unless a fixed-interval scallop fits that description. And a useful ''physical definition'' is not necessarily easily given. But here is more or less the difference in the two ''behaviorisms.'' If Baum overemphasizes the molar perspective, it is because that perspective has been largely neglected, while attention in all of psychology, including behavior analysis, has concentrated on the molecular view and the attendant mediators that view requires.

Whatever path behavior analysis takes—Baum's, Staddon's, or surely some combination—we remain a small accompaniment to the rest of mainstream psychology, largely devoted as it is to the study of topics of interest to common culture (''optimism,'' ''forgiveness,'' ''attention deficit,'' ''depression''), and largely studied through the ''dark glass'' of inferential statistics (as Staddon put it on p. 122 of his book). Baum and Staddon agree that most of psychology is a series of transient fads and that psychological research is done largely for the approbation of the public, which understands only the language of folk psychology.

They surely disagree on the importance of the molar–molecular distinction; Baum sees it as crucial whereas Staddon thinks it ''trivial'' (2004, p. 83). If Staddon's internal states are to be understood only as components of conceptual or mathematical *models* and not actual causal events inside the organism, then they are no more objectionable than other proposed intervening variables, like ''drive,'' ''value,'' and ''response strength.'' However interpreted, Baum's molar behaviorism and Staddon's theoretical behaviorism each have their value, but they will never be wholly commensurate.

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